

MEMORANDUM

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TO: Edmund Dews

DATE: 12-1-61

FROM: Daniel Ellsberg

MEMO NO.:

SUBJECT: REFEREE'S COMMENTS ON "THE COMPLETE THEORY OF POSTATTACK REORGANIZATION"
BY AVERCH, MASSELL AND WINTER

COPIES TO: H. Averch, J. A. Kershaw, A. W. Marshall, B. F. Massell, V. Taylor, S. Winter

In order better to assess the contribution of this proposed RM, I have read carefully the following related recent papers: "Postattack Damage Assessment" by Massell and Winter, RM-2844-PR, "The Sources of Economic Collapse" by Winter, D-8662, "Economic Recovery From the Effects of Thermonuclear War" by Winter, P-2416, "The Relation of U. S. Fallout Casualties to U. S. and Soviet Options" by Hamunian, P-2412, "Ecological Problems in Postwar Recuperation" by Mitchell, P-2413, and "Problems of Fire in Nuclear Warfare" by Hill, P-2414. The effect of this survey has been to confirm very definitely my initial, negative feelings towards the usefulness of issuing the present paper as an RM in anything like its present state.

In certain general aspects, other than content, this paper stacks up very badly against these others in the same general area. In particular, the two Winter papers and the Winter-Massell RM, are striking for their clarity and precision of argument, their wealth of original and stimulating comments, and their judicious remarks on the limitations and uncertainties of their discussion; this paper is not. The contrast is particularly striking since two of the authors of this paper were authors of the earlier papers; where those seem to me to be admirable models of conceptual exposition, I found myself progressively frustrated, in reading this paper, by the vagueness of concepts, the lack of essential distinctions, the broad and vaguely-hedged generalizations, and by sweeping and highly implausible obiter dicta.

If these were the only shortcomings of this paper, the excellence of the earlier work by Massell and Winter would indicate that a little rewriting effort would be all that is necessary. However, in terms of content, there turns out to be so much overlap with the earlier papers (in which the discussion is almost uniformly superior) that it is hard to conclude that the publication from this discussion would be worthwhile. The earlier papers are exceedingly rich in stimulating questions and suggestions for further research. Not only does this paper fail to answer any of those questions (or even to consider or list all of them; indeed this paper gives the paradoxical impression its authors were ignorant of the earlier work), but many of its vague statements raise questions in the reader's mind which are actually answered in the earlier papers. If these authors had not written the earlier papers themselves, I could do no better than to urge them to read those papers for more precise statements and more carefully developed discussion of the same problems with which this paper deals. As it is, I don't know quite what to say, except to congratulate Massell and Winter on their earlier work and to comment regretfully that they have not, in this case, managed to build upon it.

An important example of the vagueness to which I refer is the failure to provide criteria for the different "core economies". Although comparisons are implicit in such statements as "the largest core that can be established", "decisions as to what level core to attempt to establish" (p. 13), "the largest feasible core", "establishing a larger core" (p. 14) there is no indication as to how an ordering of cores is determined. The answer to this is not obvious, since a core is defined in terms of several distinct capabilities; would a "larger" core be one supporting the largest number of workers, or capable of meeting the larger "fixed obligation" or of generating a larger amount of net investment, or some weighted combination of these; or would only certain comparisons be possible, in terms of "efficiency"? Moreover, a thorough discussion of the possibilities of establishing "a" core economy or different sorts of core economies would require a much richer discussion than is available here of distinctive types of cores and distinctive strategies for attaining them.

Another frustrating quality of this presentation is the almost total lack of concrete examples or even hypothetical illustrations of the generalizations offered. For example with respect to the figure on p. 12, there is no attempt to suggest specific technological examples that might correspond to the points of discontinuity at A, B, and C. The earlier papers actually make far greater reference to existing empirical data and to concrete examples and possibilities.

Compare the discussion of a "subsistence loop" on pp. 17 and 18 of this paper with the much fuller development on pp. 18 to 26 of D-8662. The discussion on pp. 19 through 21 of information requirements, the need for a focus for expectations, and the inadequacy of a decentralized market mechanism in this situation, is markedly inferior to the corresponding discussion in RM-2844. Where the latter discussion presents a reasoned and detailed argument, this paper relies upon frequent usage of such phrases as "it is well known that" (see for example pp. 30, 31); and the frequent phrase "we argue that" (for instance pp. 35, 41) tends in this paper to be equivalent to "we assert". (Incidentally, the unsupported statements about the implications of centralization and decentralization lack any reference to the important work, done, as it happens for RAND, by Tom Marschak and by Arrow and Hurwicz.)

The section on "monetary and financial mechanism" pp. 33, 36 seems strikingly unrelated to circumstances following a thermonuclear war. The assertion that by using "a modified Patinkinesque version of an equation for the money market in the context of a general equilibrium system during a reorganization", "it would then be possible through appropriate monetary and fiscal policy to equilibrate the postattack economy at a level where full employment exists and total surviving capacity is utilized" pp. 33-34 seems especially whimsical. The statement that "it is possible that no type of monetary policy will equilibrate the reorganized economy" (*italics added*) seems one of many examples in this paper of unnecessarily cautious phrasing.

Finally, one almost wholly regrettable section is the one which most departs from the earlier papers, "Social Organization" pp. 39 to 43. The section begins by noting that "most discussions that these questions contain

nonoperational metaphysical propositions about society and human nature" and refers, with justice, to Kahn's "implausible psychology". But it is hard to recall in any other discussions, including Kahn's, statements of the quality of the following: "we argue here that bourgeois virtues no longer survive in the present economy", "other directed impersonalities are almost helpless without cues from their peer groups, and during and after attack peer groups will no longer exist", on the other hand, other directed types are allegedly easily manipulated through the mass medium, and, presumably, government will be able to convince people to engage in productive activity", "other directed cultures probably are more comfortable with central planning, since the economic dictator (benevolent or otherwise) will provide cues for socially acceptable postattack behavior".

These observations lead up to what is perhaps the most stunning transitional sentence ever to appear in a RAND document: "Having briefly examined the relation between postattack character and society, we now examine the philosophical or existential meaning of thermonuclear war" p. 42. The authors could easily be pardoned for failing fully to come through on the promises implicit in that introduction, but they are not so easily pardoned for following it with such propositions as: "The quality of life during recovery may differ markedly from the preattack quality. It would be impertinent to speculate on the direction of change... After a thermonuclear war, the survivors will lead a new life. It is up to them to define and pursue normality and happiness... Heraclitus noted long ago that one cannot step in the same river twice. History is much like Heraclitus' river."

Final comment: If it is the intent of the three authors to summarize in a simplified form their discussion of the three earlier papers by Massell and Winter, one could only urge them to go back to the original papers and try again. But a far more useful effort, one which would be most worthwhile for RAND to support would be to follow up the questions and suggestions implicit in those earlier papers with new research maintaining their same standards of excellence.

Postscript

In discussion with Ben Massell on my initial reaction to the proposed RM, Massell brought to my attention an earlier paper, "An Introduction to the Reorganization Problem," by Bear, Massell and Winter, D-7877, which throws quite a different light upon the present paper. I will let my earlier comments stand because it may be of some interest to the authors to compare my reactions before reading this D and after reading it.

For several reasons, after reading the Winter-Massell RM-2844-PR (October 1961), the Winter P-2146 (August 22, 1961) and the Winter D-8622 (March 29 1961), I had taken it for granted that this proposed RM was a follow-on piece; the earlier papers were on roughly the same area, all three of them are cited and quoted in the proposed RM, and the Winter D in particular promises a follow-on on the subject of reorganization. It was on this understanding that I found the current piece both surprising and disappointing. Instead of developing and extending the arguments and insights of those

papers, it seemed, paradoxically, to have regressed to a more preliminary level of discussion. Significant distinctions seemed both scarcer and vaguer; definitions or concepts which were precise in the "earlier" papers seemed less precise in this one, or were omitted entirely; generalizations were now left crying for concrete illustration, although ample illustrations had been provided in one or another of these "earlier" papers. Indeed, the whole general tone of this paper is that of a preliminary, tentative, initial attempt to establish a very broad framework for an investigation: a "first cut," setting down some exploratory comments, in the absence of any earlier hard thinking on the subject. This didn't strike me as a satisfactory approach for an RM at this stage, considering the excellent, closely reasoned work that had been done on at least some aspects of the problem "already." What was paradoxical, of course, was that this "earlier" work had been done by Massell and Winter themselves! As I remarked in my earlier comments, the defects of this piece seemed to reflect the fact that its authors had not read their own "prior" work!

The paradox is now answered; they hadn't. This proposed RM is primarily a reorganization, with some editing and extension, of D-7877 which was published on September 7, 1960, before the three other pieces mentioned above. Virtually all of the defects which I criticized, vaguenesses or inadequate developments, are related to passages which are quoted verbatim from this earlier D: which was a preliminary "first cut" in which the sketchy and tentative generalizations were not only justified but, of course, inevitable.

What remains to say is that it no longer seems satisfactory to publish that earlier D either "as is" or with the minor modifications introduced in this revision: precisely because better thought has been done on the subject, and better words written since September 1960, and that by two of these same authors! If they wish to write a paper specifically on the reorganization subject, I think that they should abandon D-7877 as a starting point (despite its seminal role) and look much more carefully at the three papers mentioned earlier for hints on the sorts of topics to be covered, the level of initial generalization, the different classes of objective situations that are "interesting," and for logical development of certain arguments.

Some examples of discussions which are, in this RM, lifted pretty much from D-7877 but which have actually been developed better by these same authors in what turns out to have been later papers: (1) on the distinction between technological and organizational problems, see D-8622, pp. 5-6; (2) on the question of our ignorance of technological input-output relations (p. 6), see the discussion in D-8622, pp. 7-14; (3) for the discussion of the "core economy," see RM-2844, pp. 13-41; (4) on the question of how long it might take to establish a core economy, that is, the length of the reorganization process, see P-2416, pp. 7-8, which brings in the dependence of this interval upon the nature of the war and the military outcome (also see RM-2844, pp. 25-26, on the question of the survival of the damage assessment system); (5) for the discussion of the risks and costs connected with different "cores," (p. 14) see D-8622, pp. 31-32; (6) for the discussion of a "subsistence loop" (p. 17), see D-8622, pp. 20-26; (7) for discussion of the inefficiency of free markets when expectations are conflicting, and the need for centralized collecting and dissemination of information, see RM-2844, pp. 18-21.

In nearly all of these cases, the discussions cited seem to me to be more complete, more satisfying, more stimulating and perceptive, than the corresponding discussions in this RM: which turns out not to be so surprising as it appeared to me at first, since these other discussions were actually written later, and represented more developed thought than did D-7877 on which this draft was based. The sections in the present paper which represent wholly new additions, for instance, that on monetary and financial mechanisms and the discussion on social organization, seem to me the least adequate.

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